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Psychological Bulletin

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JAMES R. ANGELL, University of Chicago (*Monographs*) and
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THE

PSYCHOLOGICAL BULLETIN

MATHEMATICAL VS. SCIENTIFIC SIGNIFICANCE

BY EDWIN G. BORING

Clark University

The conclusions attained in researches involving measuremen are very frequently based on estimates of identity or difference between two or more characters measured; the scientist finds that he has to deal with differences between two constants or with the divergence of one function from another.[1] Every difference or every measure of disparity that thus comes under consideration is necessarily obtained from given data with a certain measure of precision (*e.g.*, a P.E.) that gives rise to the question whether the difference is "significant" or not. When a difference is large with respect to its probable error, for example, it is assumed to be "significant," and when it is small with respect to its probable error it is said to be "less significant" or "insignificant." For this reason it is customary in the case of a simple difference between the means of two arrays to measure the "significance" of the difference by its ratio to its P.E.

It is usually held that this ratio is not a direct measure of significance, and needs to be translated into a scale of the 'probability of difference' by the use of a table of the probability integral. The values from such a table give us a series of numbers ranging from zero, when the difference is infinitesimal with respect to its P.E. (not "significant"), to unity, when the difference is infinite with respect to its P.E. ("significant"). This scale of probabilities is sometimes spoken of as the "probability that the difference is

[1] See my discussion with S. W. Fernberger: *Amer. J. of Psychol.*, 1916, **27**, 315–319; 1917, **28**, 454–459; PSYCHOL. BULL. 1917, **14**, 110–113; also *Tables for Statisticians and Biometricians*, ed. by K. PEARSON, 1914, pp. xvii f.

not due to chance." It is also used as a measure of "homogeneity" and "heterogeneity"; for, if the difference between two samples of the same data is "insignificant," the data may be thought to be "homogeneous," whereas, if the difference is large and therefore "significant," the data from the two samples taken together may be considered "heterogeneous."[1]

Pearson applies a similar principle in his "measure of the goodness of fit" between two curves. With his procedure one obtains a value χ^2 from summing the differences between two curves and, taking into account the number of cases, makes use of a table to determine the probability that the deviation of the one curve from the other is merely "random."[2]

It is a common experience of scientific persons working with human data that these formulæ frequently give values for the probability of differences that are "too high." One works, for example, with the performances of a group of women and a group of men in a mental test and one finds a "significant" difference—perhaps a probability that 99 times out of 100 the men will do better than the women,—and yet one is convinced that there is no "truly significant" difference indicated. Or one determines the deviation of an observed curve from an ideal form and finds, let us say, that only 2 times in 100 would data that tend to follow the ideal form deviate as much from the ideal as do the observed data; and yet in plotting the observations along with the theoretical form he may note that the two functions are sensibly the same, and may feel inclined (if he is not scared off by Mr. Pearson) to say that the ideal function actually does represent his data. It is with the basis for this particular scientific attitude that I am concerned.[3]

It appears that the apparent inconsistency between scientific intuition and mathematical result is not due to the unreliability of professional opinion, but to the fact that scientific generalization is a broader question than mathematical description. In scientific work we deal with samples, whereas we are always interested in the

[2] *Cf. opp. citt.*, especially *Amer. J. of Psychol.*, **28**, 451 ff., and V. Henri, *L'Année psychol.*, 1898, **5**, 153 ff.

[3] K. Pearson, *Phil. Mag.*, 1900, **50**, 157–175; W. P. Elderton, *Biometrika*, 1902, **1**, 155–163; *Tables for Statisticians*, *op. cit.*, pp. xxxi ff., 26 ff.

[4] The differences between Pearson on the one hand, and Merriman and Airy on the other, *Phil. Mag.*, *op. cit.*, 171 ff. are of this order; Pearson is statistician, and Merriman and Airy scientists. It is interesting to find Pearson shifting ground in *Biometrika*, **2**, 1903, p. 367, from a statistical result that lengths of forearm do not fit the Gaussian curve to a scientific conclusion that they do.

larger groups of which the samples are intended to be representative. The mathematical formulae do truly measure the difference between the particular samples observed. Whenever we can assume that these samples "truly" represent the total group, then the mathematical method also indicates the probability of a difference between the groups represented. A sample "truly" represents a group when the mode of variation within the sample is the same as the variation within the group at large: this is what is meant when we say we have an "unselected" sample. But anyone who has attempted to obtain "unselected" samples with human material knows what very careful selection is required to achieve this "unselected" state.[1] There are many uncontrollable factors that enter into the getting of human stuff; human beings are usually resistent to an indiscriminate mixing-up and to that arbitrary selection combined with complete ignorance of the nature of the individuals involved which constitutes "chance selection." So it happens that the competent scientist does the best he can in obtaining unselected samples, makes his observations, computes a difference and its "significance," and then—absurd as it may seem—very often discards his mathematical result, because in his judgment the mathematically "significant" difference is nevertheless not large compared with what he believes is the discrepancy between his samples and the larger groups which they represent.

It is useless to try to limit the scientist to the mere description of his samples. Science begins with description but it ends in generalization. And, since in the nature of the case it is impossible for him to state in numerical terms the degree of representativeness that his samples possess, conclusions must ultimately be left to the scientific intuition of the experimenter and his public. Such an outcome with respect to the measure of the probability of difference is not wholly satisfactory but it is inescapable. It is equivalent merely to saying that, given only approximate control of experimental conditions, only approximate results can be achieved. A knowledge of the "probability that a difference is not due to chance" is distinctly worth while on the descriptive side; but this measure of significance does not necessarily apply to the general class for which a sample stands. In certain cases it may so apply, but ordi-

[1] Statisticians' rules for obtaining "chance conditions" and "random samples," though generally failing of an appreciation of the logical truth that complete ignorance is the sole condition of chance, show how hard this particular kind of ignorance is to achieve.

narily there is a constant factor operative in the selection of human material which must be taken into account and which frequently offsets a demonstrably "significant" difference that has been made out between the samples. It is for this reason that mathematical measures of difference are apt to be too high and may need to be discounted in arriving at a scientific conclusion. The case is one of many where statistical ability, divorced from a scientific intimacy with the fundamental observations, leads nowhere.

AN OBSERVATION OF THE PURKINJE PHENOMENON IN SUB-TROPICAL MOONLIGHT

BY STEPHEN G. RICH

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The observation here reported was made at East London, South Africa, July 12, 1919. The town is in 32° S. Latitude, at the extreme southern end of the eastern sub-tropical margin of Africa. The moon was on this occasion approximately full; and at 8:30 P.M., when the observation was made, was at the zenith. The sky was cloudless, and the place of observation was beyond the area served by street-lights. The colors of objects seen by moonlight were verified by a daylight visit to the same places on the following morning.

In full moonlight a limited range of colors was visible. Reds were especially noticeable. A brick wall showed its characteristic hue; orange-red tiled roofs were plainly recognizable in color; maroon-red painted roofs were seen as a very deep brownish-red; a bright carmine letter-box appeared a dull crimson; but a dark brown-red painted roof appeared black. Greens were not all recognizable. Pine-trees appeared black; pepper-trees (*Schinus molle*), greenish-gray in daylight, were gray; but hibiscus and aloe leaves were noticeably green. My dark blue suit appeared black; the hue was identical with that of my fountain-pen held against the cloth. Yellows and violets were not observed.

In the shadow of a pine-tree all hues save a maroon-red, which became a deep brownish-red, vanished. A pair of tan shoes which I was wearing were of a dark russet color. This was clearly visible in the direct moonlight, but vanished utterly in the shadow, becoming a medium gray.

The possible interest of the observation here reported lies in the fact that the sub-tropical moonlight was exactly strong enough to give a true "twilight" vision which lasted for a considerable time. The hues that appeared most definitely when daylight vision was slightly excited were orange, red, and brown. Green appeared, but in a lesser degree.

GENERAL REVIEWS AND SUMMARIES

LEARNING

BY JOSEPHINE GLEASON

Vassar College

Pechstein had shown in an earlier paper[1] that in a motor problem certain modified forms of the part method are superior to learning by the whole. His present article (5) reports an experiment which extended to the field of learning verbatim the use of the various whole and part methods which had been applied to the maze problem. Two series of nonsense syllables were used: the A-Series of thirty-two syllables in consecutive arrangement, and the B-Series consisting of the same syllables in pairs. The whole methods were two in which the subject could retrace the material until fairly sure of his grasp, and a third in which such "returns" were prevented. The part learning was done by the "pure part" method in which the parts, first learned as units, are then connected; the "progressive part" method in which every additional unit as it is learned is connected with the material already mastered; and two "repetitive" methods in which the learned units are repeated as an introduction to the learning of the next. The results, given in terms of times, trials and errors, show that all the part methods were superior for the A-Series as they had been for the maze. The wasteful repetitive methods were inferior for the B-Series, but for both series the progressive part method is the most efficient.

When paired nonsense syllables were presented to learners both simultaneously and successively, Froeberg (4) found a difference of 12 per cent. in the percentages of right responses in favor of simultaneous presentation. This was probably due to the tendeney, greater in simultaneous presentation, to articulate the syllables and combine them into words. When colors, letters, and signs were substituted for syllables, the differences were in every case in

[1] Pechstein, L. A., "Alleged Elements of Waste in Learning a Motor Problem by the Part Method," *J. of Educ. Psychol.*, 1917, 8, 303.

favor of successive presentation. Froeberg concludes that the difference is a matter of attention: simultaneous presentation is superior only when the paired stimuli form an attentional unit. In the first experiments, an increase in the time interval between successively presented syllables from 0 to 5 seconds had no effect on the number of correct responses. When, however, the subjects obliterated their memory images of the first stimuli of the pairs by reading numbers aloud in the interval, there resulted a decrease in the number of right responses with the increase in interval.

In a series of articles (10, 11, 12) Reed discusses the relation of associative aids to various aspects of learning. As an experimental basis for the study, his subjects learned by the prompting method pairs of verbal stimuli of various types. These experiments, covering six days, were followed by additional ones which consisted in giving the pairs backward and in giving the first members of the pairs first upwards and then downwards. The first paper establishes the relation between rate of learning and rate of forgetting: "the rate of relearning varies directly as the rate of learning." The explanation of this is to be found in the greater frequency with which associations are reported to occur in the case of slowly forgotten pairs. A classification of the associations shows that with familiar stimuli they usually have a meaning basis, while with less familiar material they are likely to be sensory; and that the former are the more effective. The practice curves (11) in terms of repetitions, reaction times, and standard deviations of the reaction times for correct responses, on the one hand, and the curves for frequency of associations, on the other, have the same general shape: there is a rapid fall for the first three days followed by a gradual one for the remaining three days, and a gradual rise for the three additional tests. If associations are present on the sixth day they are accompanied by a lengthening of the reaction time. The experiment may also be taken as one in the transfer of learning: the experiments of the first six days form the training series; the additional experiments are a test series for measuring the improvement; and the learning times of the regular tests for the first day serve as a control. Since 84 to 91 per cent. of the correct responses made in the test series are accompanied by revivals of associations used in the training series, Reed concludes that there is transfer only insofar as associations established in the first set of experiments can be used in the second. Finally, (12) the experiment may be taken as a problem the solution of which involves at

its outset thinking, and gradually, with repetition, becomes a type of reflex action. The associative aids are observed to occur most frequently in the problematic stages and to disappear as the solution becomes mechanical. This means that thinking and association are not distinct processes; that thinking must rather be taken as controlled association. Reed then defends his use of objective reports—reports of meaning—as throwing more light on the dynamics of thinking than can psychological descriptions.

With a view to seeing whether learning capacity is specific, or more or less constant, regardless of material, Pyle (9) set his subjects at various tasks involving different sorts of material: substitution, learning of nonsense syllables, card distribution, and marble distribution. The records of several hundred university students give an average "raw" correlation of .50 between ability in one type of learning and ability in the other types. Pyle thinks that with elimination of extraneous factors, such as differences in previous training, the correlation would approach unity. His view is that brains differ in the ease with which bonds are established and that most learning methods involve both general and specific factors. The former are the bond-forming capacities of the central nervous system; the latter, the other characteristics of brain, sense organ and muscle.

Pyle (8) used the same test in card-distribution to observe transfer and interference of training. The compartments into which the cards were to be placed were numbered according to two schemes which used the same figures in different sequences. The subjects in group A alternated from scheme 1 to scheme 2 from day to day throughout the thirty day experiment. Group B used scheme 1 only for fifteen days and then scheme 2 only for the remaining experiments. The average distribution time of this second group for the first day with scheme 2 was only 42.8 per cent. of the average time for the first day with scheme 1. By the fourth day with scheme 2 the minimal time for scheme 1 was reached. Pyle explains this transfer in terms of such identical elements as skill in perceiving and recognizing the numbers, in handling the cards, etc. Group A's data, on the other hand, show inhibitory effects: though the initial speed is higher than is Group B's, the final speed is 10 per cent. slower for scheme 1 and 22 per cent. slower for scheme 2. The inference is that it is not economical to form two mutually inhibitory sets of habits simultaneously.

Thurstone (13) filed the records made by drafted men in re-

ceiving tests in telegraphy during their first hundred hours of practice, with the purpose of studying the distribution of ability in a group of learners at various stages of practice. The median receiving speeds for the group, and the upper and lower quartile speeds are plotted against the hours of practice. The distances between the upper and the median curves, and between the median and the lower curves at any point represent respectively the upper semi-interquartile and the lower semi-interquartile ranges at that stage of practice. These ranges, and therefore the variability, increase with practice. This variability is also shown in the more precise terms of the standard deviation, expressed in words received per second. An experiment carrying practice to its physiological limit should, by showing the relation of the standard deviation to the practice hours, make possible the use of the standard deviation for expressing a student's standing at any stage of practice. Peterson (7) takes issue with Thurstone's article on several grounds. His own curves representing the progress of students in code translation show that while variability measured in terms of attainment may increase, when measured in errors will decrease. Thurstone's inference will then hold only for attainment. Furthermore, if instead of the absolute units—the quartile and the standard deviation—their coefficients of variability are considered, Thurstone's own data show a decrease in variability.

Chapman (1) reports tests in typing passages given daily to students in typewriting over a period from the twentieth to the hundred and eightieth practice hour. Eight typical individual curves seem to show that that is no typical curve which they approximate: the short plateaus which occur do not appear at the same places in the various curves; some individuals vary erratically, while others deviate little from their true rate of performance. When the average number of correct words is plotted against the number of practice hours, the curve shows a comparatively small return for the last ninety hours of practice.

Dallenbach (2) gave to a class of feeble-minded children tests in visual apprehension which had been used with normal children. He found that with the feeble-minded as with the backward normal children the characteristic effect of practice is a slow and gradual improvement, whereas with the "medium" and "superior" groups of the normal children a large initial improvement is followed by a small one. The range of visual apprehension varies with the complexity of the material and the diversity of the associations aroused. It is directly correlated with mental age.

In his experiment in rational learning, Peterson (6) set his subjects the task of assciating the first ten numbers with the first ten letters of the alphabet, paired at random. The letters were called out in order and the subject, after every letter, guessed the accompanying number until he hit upon the correct one. He was free to choose any methods occurring to him. Peterson classifies the errors made as logical and perseverative. The former consisted in giving a number which had already been used for an earlier letter; the latter, in repeating a wrong guess in response to a single letter. Such errors are to be attributed to the excitement attendant upon test-taking for some subjects, and to a limited distribution of attention among the essential factors of the experimental situation. The error curves do not differ radically from other learning curves, though in only the better of the records are there the significant drops which characterize the curve in maze-learning. Peterson suggests that such tests may promise something in the way of analysis of individual traits.

Dearborn and Brewer (3) give the results of a class experiment in the effects of practice. The practice consisted in translating long prose passages into the old Civil War code used in several mental tests. The subjects had to construct the code mentally, after the preliminary explanation, and were given five-minute practice twice a day for two weeks. Before and after the practice series they were given a group of five tests sufficiently similar to the practice exercise, it was intended, to be reinforced or interfered with by the improvement made in it. The tests selected were one in digit-symbol substitution, a complex dotting test, a number code, a test in digit-letter substitution, and a letter code. The scores of the practiced group in the first and the final practice periods were compared, and their test scores were checked by the scores of two control groups who took the same tests. There is a small positive correlation between the first and the final scores in the practice series. There is also a small positive correlation between gain in practice and gain in the tests, with the exception of the test in the letter, where a small negative correlation indicates interference.

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MEMORY AND THOUGHT

BY W. C. RUEDIGER
George Washington University

Berliner (3) reports two brief series of experiments on the time relations and quality in the recall of simple visual images. In the first series observations made in the morning and at night were compared and showed a distinct tendency for the images to be held longer in the morning, while the effects of the day's mental work upon the time required to call up the image, and upon the quality of the image as recalled, were less marked. Eight persons participated in the experiments, from three to five days each. The second series used an interval of half an hour of intensive mental work (addition of figures), and gave about the same conclusions. In this series only two persons took part, in one day.

In the field of the relation of the speed and permanency of learning, Gates (9) reports a study on the correlations of immediate and delayed recall. His subjects were elementary school children and his tests were based independently on nonsense syllables and connected sense material, the final results of the two types of tests being averaged. He found positive correlations as follows: (1) from .73 to .89 between amount recalled immediately after learning and amount recalled three or four hours later; (2) at least .50

between amount recalled immediately and the *proportion of that amount* recalled after the interval; and (3) .50 between the estimates of intelligence of the pupils furnished by the teachers, and all the tests of immediate and delayed recall. Between the various single tests of the two types of material he found a correlation of +.50.

With the purpose of (1) making a beginning in establishing the associational basis of English and (2) of comparing the results found in English with those found by Thumb and Marbe in German, Esper (**6**) reports an experimental study on word associations. He used 126 observers divided into three groups: 100 university students, 11 children, and 15 university janitors. As stimulus words, he used 10 names expressive of family relations, 10 adjectives, 10 numerals from 1 to 10, 10 pronouns, 10 participles, and 10 adverbs of time and place. Only one- and two-syllable words were used to avoid the possibility of prolonging the reaction-time. Reaction-times were measured with the Hipp chronoscope. From the results obtained the following conclusions appear to emerge: 1. The rule established by Thumb and Marbe, namely, that the more frequent an association is, the more rapidly does it take place, is confirmed by the English-speaking observers. Thus, out of 95 successful reactions to the stimulus word "father," 69 were "mother" with a median of 1.160 seconds; 4 were "son" with a median of 1.248 seconds, and 22 remaining cases with a median of 1.904 seconds. 2. In both languages, words of a given category are associated predominantly with words of the same category. (*a*) In all categories investigated, with the exception of the numerals, reciprocal associations were found. In these cases, a word *A* which calls up a word *B* is in turn called up by *B*. (*b*) Numerals are associated predominantly with higher numerals. (*c*) Adjectives are associated predominantly with adjectives of opposed meaning. 3. A comparison of the English and German results tends to show that the associations of English- and German-speaking communities correspond in the case of most words which are of familiar meaning and in universal use in both languages. 4. The reaction-times of the associations of children and uneducated adults are longer than those of educated adults, but the favored associations are in most cases the same and the essential character is similar.

Foucault (**8**) in an experimental study on forgetting, concludes that the formula presented by Ebbinghaus is too complicated and lacks the uniformity that is demanded, and that the attempt by Meumann to revive the formula of Ebbinghaus fails through

neglecting the influence of the time of day on the fixation of knowledge in the mind. His own results are presented so abstrusely that little can be made of them.

Arreat (1) revives the old question of whether we have one memory or distinct forms of memory. To accept the latter alternative he thinks implies the acceptance of the old doctrine of cerebral localization, and as he rejects this, he is forced, or thinks he is, to the conclusion that we have one memory. He does grant the possibility of distinguishing between a memory of perceptions and a memory of emotions, but beyond this he considers memory a unitary function whose application by different individuals is very diverse.

Under the title of "Esthetique et Memoire," D'Eichthal (4) points out the relation of memory to the appreciation of the beautiful. His theme is that the appreciation of music depends on the memory of the notes which are past. The repetition of a certain theme is for the ear the return of an old acquaintance. The pleasure derived from oratory and poetry is due to the rhythmic repetition of accents or rhyming syllables. Painting, architecture, sculpture—all depend upon symmetries, repetitions, and equilbriums which are carried in the memory as the appreciative gaze passes over the whole. This is all very good; memory undoubtedly does function in esthetic appreciation; but when D'Eichthal explains individual differences in esthetic appreciation by differences in memory he oversteps his bounds.

The present day conception of logic, according to Avery (2), differs from the traditional one in regarding logic as the science of relations instead of the science of the laws of thought. The term "thinking" has a subjective or psychological reference which, in the present interpretation of logic, should not be emphasized. The older view appeared to place logic within the field of psychology. If logic is defined as the science of relations, then, obviously, the types of relations met with must be distinguished. These Avery divides into (1) transitive and intransitive and (2) symmetrical and asymmetrical. A transitive relation is exemplified by:

A is to the right of *B*,
B is to the right of *C*.
Therefore *A* is to the right of *C*.

An intransitive relation is exemplified by:

John is the father of Henry,
Henry is the brother of William.

As the relation is intransitive, it cannot be concluded that John is the father of William.

A symmetrical relation is exemplified by $A = B$, and an asymmetrical one by A is B. In the former, one may go either forward or backward but not in the latter.

After reviewing the most prominent attempts made during the last century to give a logical division of judgments, Lodge (**11**) concludes that modern logic recognizes no logical division of judgments into coördinate types and species and that judgments cannot be divided on a logical, but only upon a psychological, basis. Taking, then, the psychological basis of the predominance of sensory and intellectual elements, he presents the following division: judgments of (1) perception, (2) experience, (3) symbolic judgments and (4) transcendent judgments. A judgment of experience differs from the perceptual judgment in that it depends more on memory or previous perceptions than on direct present perception. The symbolic judgment differs from the judgment of experience in that it extends our knowledge beyond the field of actual experience. The transcendent judgment is an attempt to extend the field of symbolic judgment beyond the limits of human experience, actual or possible.

Wells (**12**), in a somewhat controversial article, maintains the thesis that "the biological foundations of belief may be exhibited in two ways. In the first place it may be shown in what manner some of the human instincts, which are the basis of man's emotions and desires actually determine his beliefs. Since the instincts exist as one outcome of the biological struggle for life, so far as beliefs rest upon instincts they rest upon biological foundations. In the second place, attention may be called to the direct survival value the beliefs possess through their 'subjective' effects upon the physical economy of life. . . I contend that beliefs need not always be true to be valuable."

Edgell (**5**), Fawcett (**7**), and Johnson (**10**) have papers in *Mind* for 1918 whose titles suggest psychological contents but whose actual contents do not bear out this suggestion. The titles are respectively "The Implications of Recognition," "Some Observations Touching the Cosmic Imagining and 'Reason,'" and "Analysis of Thinking." The first two of these are out-and-out metaphysical discussions, of interest only to professional metaphysicians, while the third analyses thinking into a host of abstruse verbal distinctions having linguistic rather than psychological implications.

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SPECIAL REVIEWS

Some Questions of Phonetic Theory. WILFRED PERRETT. (Chapter V. The Perception of Sound.) Cambridge: W. Heffer & Sons, 1919. Pp. 39. 2 s.

The reviewer regrets that he has not had access to the previously published chapters of this work. The chapter under review is an argument against the theory of resonance as advanced by Helmholtz. After a brief consideration of the historical antecedents of the Helmholtz theory, the author proceeds to discuss various data from acoustics and phonetics which point to the conclusion that the movements of the internal ear are *dead beat* in their character.

The conception of resonance is replaced by that of *synkinesis*, a kind of sympathetic vibration in which no natural resonance tone is involved. The diaphragm of a telephone or of a phonograph acts synkinetically "being aperiodic in relation to the periods of the vibration transmitted, beginning and ending its vibratory motions accurately with those vibrations, without awaiting any summations of impulses before it can get into its swing—having no swing to get into—and without continuing to execute extra vibrations after the force which moves it has ceased to act."

The chief objection to resonance in the internal ear concerns the continuance of audibility by virtue of this resonance which, according to Helmholtz, must involve some such period of time as that occupied by 9.5 vibrations of the tone. Since it requires a certain summation of vibrations to bring the resonator into its full swing our ability to hear a note beginning at full strength is unexplained. Furthermore a low note would require a longer time than a high note. Two sounds must synchronize to within 1/100 of a second to be perceived as simultaneous, and a single vibration of any bass note below G takes a longer time than that.

The crucial instance advanced by the author in opposition to a theory of resonance concerns the "voiceless occlusives." A graphic record of the word *utter* shows that the time during which the tongue forms the t-closure is shorter than would be demanded by the resonance of the preceding sound. If resonance obtained it would be impossible to distinguish *utter* and *udder* since the distinction rests upon a reduction of the sound to silence between the two

syllables of *utter*, and with a resonating sound no such silence could supervene in the brief period which the voice actually employs. Indeed all voiceless occlusives such as *p*, *t*, and *k* in "stop, please," "not to be" and "book-keeping" would be impossible on the hypothesis of resonance, because internal vibrations must continue the sound for a brief period until damping is effective. "It is precisely the *absence* of resonance that make the voiceless occlusives possible."

The author contends that Helmholtz should have rejected the hypothesis of resonance when in his fourth edition (1877) he ceased to regard the otoliths as the occasion of noise. But some recent investigations, notably those recorded by the physiologist Parker[1] and the physicist Stewart,[2] have brought forward evidence that perhaps the cochlea is not the only seat of hearing. If noise is not a phenomenon of resonance, a crucial instance for the complete overthrow of the resonance hypothesis must be sought with reference to the sensation of tone.

Perrett's discussion, though seemingly well informed, is marred by flippancy of style and the frequent introduction of "smart" terms of speech. The effectiveness of the argument is thus seriously impaired by an egregious trifling.

R. M. Ogden

Cornell University

Theorie der Konsonanz und Dissonanz. (*Schriften zur Anpassungstheorie des Empfindungsvorganges.* 2 Heft.) Julius Pikler. Leipzig: Barth, 1919. Pp. 34.

In place of the excitation theory (Erregungstheorie) of sensation, the author advances a theory of adjustment (Anpassungstheorie), in accordance with which the apprehension of sensation involves a relational activity on the part of the mind. In the phenomena of consonance the author traces the effects of an adjustive attitude which is but an expression of the general principle of self-preservation. With this principle as a foundation he proceeds to distinguish sharply the method of apprehension on an arithmetic basis from apprehension on a geometric basis; the latter possessing the natural significance which endows musical consonance with its peculiar effectiveness, since it consists in measuring all tones with reference to a fundamental.

Just as a comparison of lines visualized is furthered and made

[1] G. H. Parker. A Critical Survey of the Sense of Hearing in Fishes. *Proc. Amer. Phil. Soc.*, 1918, 57, 69 ff.

[2] G. W. Stewart. Binaural Beats. *Psychol. Monog.*, 1918, 25, No. 108, pp. 31 ff.

pleasing whenever one line reappears as a fractional part of the other—so that the one may be said to involve the other—two tones are consonant whenever one of them recurs in the other. This is most notably the case in the octave where the upper note is essentially a doubling of the lower note. But it is also the case in the fifth where the upper note is analysed as the lower note plus one-half ($1 : 1 + \frac{1}{2} = 2 : 3$), and in the major third where the upper note is again the lower note plus one fourth ($1 : 1 + \frac{1}{4} = 4 : 5$). Beyond this the consonance of a single interval does not extend. Although the same principle would seem applicable in the case of the whole tone, $8 : 9$ and the quarter-tone, $16 : 17$, as a matter of experience we fail to hear these intervals geometrically, because with such small differences, an arithmetic means of apprehension appears to be easier. The fourth, although essentially dissonant, may possibly be heard geometrically as half the fall of the higher to the lower tone of the octave; similarly the minor sixth ($5 : 8$) may be apprehended as a fall from the upper to the lower tone diminished by a quarter. The apparent consonance of the minor third and the major sixth are explained with reference to their positions in triads. For example, the minor third, when taken singly is dissonant, but it becomes musically useful in combination with the major third and the fifth where it occurs as a natural consequence of combining these two consonant intervals. It is, however, a mistake to regard the triad as a fundamental element in consonance, for in reality it is only the simplest and most immediate means of resolution for a dissonance.

In order to deal justly with Pikler's theory one should doubtless consult his larger work (*Sinnesphysiologische Untersuchungen*) to which frequent references are made in the pamphlet under review. Unfortunately this book is not yet available to the reviewer, and therefore no extended criticism of the foundations upon which his theory of relational apprehension rests, is here possible. One may venture to say, however, that although the author refers in all cases to the immediate experience of intervals as the basis of his conclusions, and thus even goes so far as to exclude the fourth and the major sixth from the ranks of consonance, although hypothetically they might be justified as representing one third and two-third "doublings" of the fundamental, there is no indication that he has analyzed his tones so as to inform us just what is "doubled" in the octave, or "half-doubled" in the fifth. The analogy with the visual apprehension of straight lines of different lengths is hardly convinc-

ing unless we know what precise dimension or attribute of the tone is under consideration in judgments of consonance. The application of geometrical and arithmetical conceptions is apt to be more confusing than helpful unless it be first grounded upon judgments of liminal variation. And although it has been demonstrated that the discrimination of pitch tends to follow an arithmetical series of vibrational increments while discriminations of volume follow a geometrical series, it by no means follows that either the pitch or volume dimension of the sound can be quantified. While all would agree that the two tones of an octave possess a common quality, to go farther and state that this common feature or *Selbigkeit* is explained by the fact that the upper note doubles the lower one appears to the reviewer somewhat naïve.

R. M. OGDEN

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The Psychology of Musical Talent. C. E. SEASHORE. Boston: Silver, Burdett, 1919. Pp. xvi + 288.

"This monograph is addressed to students of applied psychology. By content, it appeals directly to those who are interested in music." It is a presentation of "the psychology of musical talent, an account of the musical mind. This subject is treated from both the theoretical and the practical point of view, the aim being to describe and explain the musical mind in such a way as to serve in the recognition, the analysis, the rating, and the guidance of musical talent." "Our approach is that of experimental study."

If we turn to the body of the book, we find that the first chapter gives the following inventory of the factors in musical talent: I. Musical sensitivity, which consists of simple forms of impression, —the senses of pitch, intensity, time, and extensity; and complex forms of appreciation,—the senses of rhythm, timbre, consonance, and volume. II. Musical action,—the natural capacity for skill in accurate and musically expressive production of tones in the control of pitch, intensity, time, rhythm, timbre, and volume. III. Musical memory and imagination, which includes the subheadings auditory and motor imagery, creative imagination, memory span, and learning power. IV. Musical intellect, consisting of musical free association, musical power of reflection, and general intelligence. V. Musical feeling, comprising musical taste, emotional reaction to music, and emotional self-expression in music. In so far as experimental technique has developed sufficiently,

each of these capacities is measured and recorded for each individual. Graphs are then made which show at a glance how the talents of the particular person under consideration are distributed. This inventory is the nucleus around which the remainder of the book is built. Chapters II–VIII inclusive are devoted to musical sensitivity. Throughout these chapters norms are given for discrimination of differences of pitch, differences of intensity, differences of temporal interval, and differences in degrees of consonance. Instructions are given showing how to convert the records of individual performances into percentile rank-order by means of the ogives which accompany the norms. No norms are shown for rhythm, timbre, extensity, and volume, but methods are suggested for obtaining them in rhythm and timbre. Seashore thinks that norms for extensity are unnecessary because "extensity varies exactly parallel with pitch," and that they are not needed for volume because it is expressed in terms of extensity, intensity, and timbre. But this doctrine takes no account of the limens for pitch and volume, (Seashore's "extent"), reported by Rich[1] which indicate that pitch and volume are not exactly parallel. If the work of Rich is to be accepted, then it would be necessary to establish norms for extensity independent of pitch, and for volume in so far as it is conditioned upon extensity. From this point Seashore goes on to discuss methods of measurement of the highest audible tones and the lowest audible tones and measurement of simple acuity throughout the whole tonal range. The convictions of the author concerning the relations of these fundamental "senses" to age, sex, general intelligence, and training are set forth, with tables in certain cases showing the distribution of the factors under consideration. There are also paragraphs upon the musical significance of these same "senses." The foregoing are the principal systematic discussions, but there remain comments upon the physical basis of pitch, the physiological basis of pitch and intensity, individual differences in pitch discrimination, illusions of pitch, periodicity in sound and other matters which need not be discussed here.

Chapter IX, on voluntary motor control, describes instruments and methods for measuring motility, timed action, reaction time, discriminative action, serial action, precision in movement, and strength and endurance in action. Chapter X, on musical action, shows the relation of the capacities discussed in the foregoing

[1] Rich, G. J. "A Study of Tonal Attributes." *Amer. J. of Psychol.*, 1919, **30**, 121–164.

chapter to control of pitch, intensity, time, rhythm, timbre, consonance, and volume. There is a description of the tonoscope and a summary of the results obtained when a singer watches himself sing and uses his eye to correct errors which his ear is not sensitive enough to perceive. Chapter XI contains the presentation of methods for describing and measuring imaginal types. There is an ogive for conversion of ratings into percentile rank. The importance of the role of auditory imagery in music is stressed; and it is emphasized that "auditory imagery is almost inextricably tied up with motor sensations and motor imagery." The chapter ends with descriptions of the types of musical imagination and with a presentation of the significance of imagination as a musical talent. The experimental part of the chapter on musical memory is a graph and an ogive of norms for ranking auditory memory span for pitch. There is also a sketch of the method for obtaining the norms and individual values. The theoretical part suggests a method for measuring retention, one for measuring the rate of learning, and one for measuring absolute pitch. The three final chapters, on musical intellect, musical feeling, and the individual and training in the art, are theoretical.

The author's purpose in this volume is to promote the vocational and avocational guidance of musical talent. It will be granted that the end is praiseworthy. The debate lies in the degree with which this purpose has been fulfilled. It is unsatisfactory to find that questions disputed in the literature are passed over dogmatically, unless such treatment can be justified by the popular nature of the presentation. There is conspicuous difficulty with the systematic significance of the capacities listed as elementary. A capacity is elemental when it "does not vary with training, intelligence, or age after the child is intelligent enough to observe." Not all the capacities which we are told are elemental can qualify under this definition; the sense of time, for instance, varies with intelligence. A more serious difficulty lies in the fact that there is no convincing evidence that the sum of the elementary capacities will combine into musical talent. When the separate capacities are measured, the best that can be said is that the individual measured has proficiency of such and such a degree in capacities that a musician finds useful. The measurements say more nearly that this person could be a performer in music if he had the inclination; but proof is lacking that he is a musician. Although the significance of the results may be questioned, it is true that the

suggestions of method for obtaining more results, and the anticipation of the significance which these results would have when obtained, are positive incentives to complete the author's elaborate program.

H. G. BISHOP

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Filosofien I. Norden. ANATHON AALL. Kristiania: Jacob Dybwad, 1919. Pp. vi + 378.

In the above work the scientific world is offered for the first time a comprehensive and valuable critical history of "new thought and mental science," in the four countries of the North: Sweden, Denmark, Finland and Norway.

The first chapter of the book deals with modern Swedish philosophy. Here we note contributions to general philosophy made by Swedish thinkers like Rudbeck, Bilberg, Rydelius, Wallerius, Linne, Svedenborg, and especially Chr. J. Boström, who according to Dr. Aall, was for Sweden what Kant was for Germany, Comte for France, and Spencer for England. Among modern Swedish thinkers Dr. Aall mentions H. Larson, Nilson, Vannerus, John Landquist, and others. Much emphasis is placed, however, on modern empiricists and contributors to experimental psychology. Here we get acquainted with scholars like Sjögren, Herlin, the physiologist Oehrwall, Thunberg, Gullstrand, Lundborg, Bjerre, Gadelius, Alrutz, M. Jacobsson, Hammer, Jaederholm, and others.

As to Finland, Dr. Aall finds a similar mental interchange between that country and Sweden as has been existing between England and the U. S. The Finnish contributions show a more exclusive philosophical trend.

In Denmark, a general characteristic of philosophical development is to be found in a never-ending fight against theology, through men like Holberg (Norwegian born), Orsted, Dreier, Kierkegaard, Brochner, G. Brandes, H. Höffding, Buch, and Thomsen.

Dr. Aall gives very interesting original views on Ludvig Holberg, the well-known author who in the latter part of the eighteenth century founded Norwegian Danish individualism. Niels Treschow and the brothers Orsted are given much consideration, but perhaps the most outstanding silhouette in Danish philosophy is shown to be Sören Kierkegaard (1813-55), the man who fought Hegelianism and founded his own peculiar system of individualism

and whose great influence on Scandinavian and European thought reveals itself more and more clearly the more it is studied. H. Höffding's philosophy is well known in this country, but also here Dr. Aall gives new points of view. For Danish experimental psychologists, the work of Alfr. Lehman and his school (Buch, R. H. Pedersen and others) is well expounded. We learn that Lehman's early investigations on color, for instance, meant an attempt to show that Fechner was right in the fundamental principles of his physiological esthetics. On the whole Lehman stands out for us as a psycho-physicist, of the highest sincerity and originality.

Philosophy and psychology in Norway has a broad place, and has got an interesting treatment on the background of the development of the other sciences and literature, etc., of the country since 1814. Philosophers like Treschow, and "the champion of Hegelianism" Monrad, show that Norwegian scholars up to 1900 were influenced by English, but more strongly by German thought. The reader gets a strong impression of the bearing of Norwegian literary geniuses upon philosophy. The chapter on "Henrik Ibsen, a Norwegian philosopher," furnishes in itself a strong and original thesis. Through A. Löchen and Kr. Birch-Reichenvald Aars, modern experimental psychology got its start in Norway (about 1900). Since then introspective and psycho-physical studies have constantly taken more and more of a foothold, and the author himself has gathered around him a school that takes the strictly empirical and experimental point of view. Quite a series of genuine contributions has appeared, mostly in German. Although it is frequently said that German influence has been predominating in Scandinavia, Dr. Aall's work shows that many inspirations have found their way *into* Germany from the North, and that there has been also a close connection with France and England.

The author gives hundreds of minute references distributed throughout the text; among these American science seems to be drawn in through Pierce, Münsterberg, and William James, who is mentioned twelve times. The hitherto altogether too little mental intercourse between Scandinavia and this country is to be regretted and is perhaps finding its remedy right now, in the growing interchange of students.

With the interest of the scientific North now turning westward as never before, due consideration will speedily come to the great and inspiring work being done in America. Considering the vast-

ness of the field covered, through constant illuminating reference to Western Europe at large, Dr. Aall's work marks a source book for psychologists, philosophers, educators, scholars in literature and art, and others, who desire to get a scholarly view over the Scandinavian field, written by a man who for a lifetime not only has himself given original contributions to psychology and philosophy, but who also has for decades expounded the progress of Scandinavian science through representative European periodicals. No doubt an English translation of Dr. Aall's work would be heartily welcomed by a vast majority of Anglo-Saxon scholars.

MARTIN L. REYMERT

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A Study of the Mental Life of the Child. H. VON HUG-HELLMUTH. (Trans. by J. J. Putnam, & M. Stevens.) Washington: Nerv. & Ment. Dis. Publ. Co., 1919. Pp. xiii + 154.

This book is interesting and in many ways suggestive to students in child psychology. When Dr. Hellmuth, however, undertakes at at the present stage to tell all about the complex "mental life" of the child, in the exclusive light of Freud's psychoanalytical mechanisms, it is a little pretentious. The book is more a philosophical discourse than one which has to do with psychology proper. Child psychology is some decades old; several centuries hence an encyclopedic work like the one Hellmuth has attempted may be written. The chapters comprise among others: "The Functions of the Senses in the Service of the Affective Life of the Infant," "The First Signs of Volitional Activity," "Development of Intellect," "Beginnings of Speech," "Development of Ethical Feelings," "Understanding," "Memory," "Imagination."

The genetic points in the treatise are valuable but offer very little new outside the attempt to get Preyer, Darwin, Shinn, Scupin, Sully and others in under the Freudian view and by drawing examples from these wherever such examples will fit in, a procedure which, according to Dr. Putnam and the author, shall excuse psychoanalysis from the criticism of "using only ex parte data"! Miss Hellmuth sees only "The Freudian Child" without reference to the improvements given the psychoanalytical method by Jung, Adler, Ferenczi, and others, through whom the present reviewer has great hope for the future of psychoanalysis, especially within the intricate field of children's emotions. However, psychoanalysis must rid itself of dealing in generalities—of the *a priori* big sweeping

statements—often based on a few selected facts, if it wants to rise from a "method of healing" into an objective science, and Hellmuth's book offers a new proof of the conclusion that this change seems yet to be far off. It is true that other child psychological methods at present share in this general fallacy—we seem to know all about "the child" and very little about individual differences, which for all our wisdom at this time may outweigh any "general laws"! With behavioristic, psychophysical, introspective, and other methods, psychoanalysis may, by laying bare the individual child, become of great service, but then it must turn from speculations like Hellmuth's into minute experiments and controlled observations. The German original of the book is not given, so the translation can not be judged. Is it good form that the names of the translators head the pages, instead of that of the original author?

MARTIN L. REYMERT

UNIVERSITY OF IOWA

The Psychology and Pedagogy of Anger. R. F. RICHARDSON. (Educ. Psychol. Monog. 19.) Baltimore: Warwick & York, 1918. Pp. 100.

This is a study of the conditions, course and consequences of anger as they appear to introspection. Twelve persons, ten of them graduate students of psychology at Clark University, reported daily to the author from notes during a period of three months or more the cases arising in their own experience. Their findings and the author's comments, together with certain pedagogical applications, make up the volume. As might have been expected a good deal of difference was discovered in different cases in the same individual and in the responses of different individuals according to their temperament and previous training, one important conclusion being that, so far as the reaction is due to individually acquired habits, it is capable of being trained. The chief place of anger itself in education is found in the fact that it is often a stimulus to effort in the desire to vindicate one's own powers of achievement and win esteem in the sight of others. Would that the author might take the lesson to heart, provoked by the knowledge of the irritation produced in one reader by the looseness of his writing and the carelessness in the proof-reading!

H. N. GARDINER

SMITH COLLEGE

Le subconscient normal: An Experimental Study of the Cognitive and Affective Processes in Retention and Recall. EDOUARD ABRAMOWSKI. Paris: Alcan, 1914. Pp. 442.

This study, printed in 1914 at the declaration of the war, was not issued until 1918.

The experiments in Chapter I show the part that memory plays in the processes of comparison and recognition. A puzzle design, made up of seven colors of irregular and unusual shapes, is presented to the subjects for varying durations and under varying degrees of distraction. The distraction is produced by requiring arithmetical calculations at various stages in the learning and relearning processes. Introspections were required and the types of errors were classified. From these experiments the author concludes that in all presentations after the first, the judgment of identity appears immediately. New details are not perceived as new but as things previously and indistinctly seen. In the series with distraction, the judgment of identity is present even though the details are not discriminated. The mass of undifferentiated details form a "general impression." In addition to the judgment of identity and the general impression, the author describe a third factor, the consciousness that there is "something" below the threshold of consciousness. The subject *knows* that his reproduction is incomplete. The forgotten details lie below the threshold, are near, but nevertheless inaccessible for thought. If the effort to remember is prolonged, a state of irritation or annoyance develops because of their anonymous absence. There is, so to speak, a resistance against accepting tricky or false memories. Genetically, the author regards memory as made up of: (1) An undifferentiated, non-representative, non-cognitive, *affective* factor that can only be described as a consciousness of something missing in the fidelity of the reproduction, and a feeling of familiarity in the recognition. There are four degrees of this affective consciousness: (*a*) the lowest degree manifests itself as a vague consciousness that something is lacking, (*b*) a feeling, non-cognitive and immediate, that something specific is lacking, (*c*) a specific feeling of the familiarity of the design, (*d*) imagery, highly emotional in tone and having a symbolic reference. (2) A *partial image* that gradually develops under the activity of cognition and which varies with the degree of attention. These partial images which are felt to be symbolic, become vivid when cognition is arrested by an emotional disturbance.

The author regards memory as an affective phenomenon,

partially intellectualized by imagery. Recognition is described as the perception of an object under its non-cognitive aspect.

In Chapter II are reported experiments on the influence of duration on the recognition of forgotten words, immediate memory for words, effects of distraction. Hallucinations, paramnesia, memory illusions, perception, are then interpreted in the light of the results. Three general conclusions seem warranted: (1) In each perception there are two elements, (*a*) the pure impression and its elaboration during the act of attention, (*b*) an affective content, of which the verbal equivalent is the phrase, "There was something." The act of attention transforms this something into an object that may be named, identified, and classified. (2) When a percept is being forgotten, memory of it is reduced to its affective content. (3) Everything that is consciously forgotten, exists in an integrated and affective subconscious condition. This is the self, the individual; a unified continuum persisting through all the variations in the conditions of life, health, and intellect.

In Chapter III are reported experiments designed to show how the normal subconscious is developed. Illustrated post cards with varying number of details were presented with and without distraction. Reproduction followed after controlled intervals. The normal subconscious is described as a selective and integrative unit that presents different degrees of psychical vitality and of dissociation, depending on the original intelligence, and which in its latent condition exhibits two simultaneous but inverse tendencies: one directed toward raising the subconscious elements above the conscious threshold, the other toward depressing them still further below the threshold.

In Chapter IV the experiments are directed toward showing the relation between the resistance against forgetting, and the affective tone of memory. If a series of words is imperfectly memorized, memory gaps will be experienced in reproduction. With respect to cognition these gaps may be blank, but when a false term is suggested for the forgotten term, there is experienced a resistance against the false term. This phenomenon the author designates as the positive resistance of the forgotten. Not only are the memory gaps *felt* but the feeling is more or less specific in the sense that the forgotten element seems to belong to some particular class or group, even though the specific element cannot be recalled. It is this feeling of group or family of a percept that is the cause of the resistance against accepting false substitutes for the terms

actually forgotten. When the forgotten element is emotionally significant, a false suggestion may be accepted as true, and this is described as negative resistance.

In Chapter V the experiments are extended to the tactual and muscular senses with practically the same results.

In Chapter VI, psycho-galvanic measurements are reported and from a consideration of the results the author concludes that the "feeling of the forgotten" varies directly with the degree of affective coloring of the words presented. The effect of doses of alcohol show a decrease in the affective tone.

Chapters VII and VIII are devoted to reproductions of well-known paintings with a view toward investigating the esthetic feelings, the feeling of "strangeness," the nature of telepathy.

In Chapter IX the author formulates a new theory of memory of which the following seem, to the reviewer, to be the salient features: (1) The ordinary state of the total past is a *feeling* of the entire mass of the forgotten, and this is the feeling of individuality. We may regard it as latent memory, the subconscious, or the cryptamnesia that accompanies all consciousness. (2) The group feeling of certain forgotten facts becomes organized around a sensation or around some symbol in some way. It appears in vague affective memories and often in our artistic productions, but we find it also in our ordinary memories as the substratum for the recall of specific periods of our life. The totality of these group feelings make up the "drives" of the subconscious. They play an important part in psychopathology and at the same time they form the basis of the normal association of ideas. (3) The simplest element of latent memory is the group feeling (of a forgotten particular element) that manifests itself in relearning as a feeling of something missing, and in recognition by a resistance against false suggestions. These three aspects of affective memory represent the subconscious part of the mind.

Considered as a whole, the reviewer believes this work will prove of value to the clinical psychologists. The experimental work is ingeniously handled and the applications to psychopathology are carefully worked out. The work may well be designated as an experimental study of the characterology of the "censor." The experimental psychologists will probably maintain that the various factors enumerated by the author have not been reduced to sufficiently simple mental or behavior elements.

A. P. Weiss

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De l'inconscient au conscient. GUSTAVE GELEY. Paris: Alcan, 1919. Pp. 346.

In the preface the author calls attention to the fact that in the explanation of life phenomena, *chemical* processes are emphasized when the study is from the lower to the higher forms, but when passing downward from the higher toward the lower, *consciousness* seems most significant. Neither classical biology nor classical psychology has given a satisfactory explanation of the relation between organic and psychical phenomena. Beginning the work with a critical analysis of the theory of evolution, the author concludes that neither the Darwinian nor Lamarkian interpretations of the method of evolution can account for the origin of species, the origin of instincts, the appearance and permanence of mutations. It seems hopeless to regard the individual as being the product of purely physico-chemical processes as these are usually defined. It seems necessary to postulate other forms of energy that exert a directive or creative influence upon the physical and organic world. The laws that govern the physical world are not rigorous and absolute as we have believed. They have a relative value and may be temporarily or accidentally modified or suspended. The author then calls attention to the profound changes that take place in the metamorphosis of an insect through the larva, pupa and imago stages. These changes are too uniform and striking to be regarded as resulting from known physiological processes unless there is a directive determiner of some sort. The analogy of insect metamorphosis is then carried over to some striking materialization phenomena in a medium by the name of Eva. The apparent similarity of these materializations of hands, faces, etc., to embryological development, have led the author to a super-physiology and a super-psychology. As super-psychological problems are mentioned: thought reading, telepathy, visions, spiritism. Super-physiology is the physiology that underlies these phenomena. Both super-physiology and super-psychology condition each other reciprocally. The organic complex, its physiological functions and all vital processes are conditioned by a superior force. The concrete notion of this force should be subsidiary to the abstract idea of its creative or directive capacity.

The classical conception that the self is a synthesis of conscious states, or that it is the functioning of the central nervous system, must be abandoned. The origin of the subconscious capacities are not sensory but originate in inspiration, intuition, genius, and

are entirely independent of learning or acquired capacities. There is in life a psychic force that is not conditioned by the organism and which force constitutes the essential character of the self. This psychic force passes by evolution from the subconscious to the conscious, and instinct is to be regarded as only this subconscious ideoplastic force functioning under favorable conditions. A large part of consciousness normally remains latent, but the psychic force of the subconscious tends to become the psychic force of the conscious. In the final stages of evolution the subconscious will disappear and only consciousness will remain.

The interest of the author would lead the reader to expect the re-interpretations of the various philosophical theories that are found toward the end of the book. Much space is devoted to Bergson, but Schopenhauer and de Hartmann are not neglected. More concrete consideration is given to normal and abnormal psychology, neuropathic conditions, neurasthenia, hysteria, mania, hypnotism, dissociation of personality, mediums.

The reviewer believes that this work will be enthusiastically received by those disposed to follow the works of Conan Doyle, Sir Oliver Lodge, and Professor Hyslop. For the majority of the professional psychologists of America the relation between normal psychology and normal physiology is baffling enough, and the acceptance of a super-physiology and a super-psychology will probably meet, in the words of the author, with considerable positive resistance.

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Experiments in Psychical Research at Leland Stanford University. J. E. Coover. Stanford University, 1917. Pp. xxiv + 641.

In 1912 Thomas Welton Stanford, a brother of the founder, made Stanford University a bequest of £10,000 to endow a fellowship for psychical research. The volume under review is the first extensive report of the experimental work which has been undertaken upon this foundation. After a foreword by the Chancellor of the University, David Starr Jordan, and the author's preface, there follows an introduction by Professor Frank Angell, describing the inception of the foundation and its program of work. The body of the volume is divided into five parts, and five appendices.

Part I deals with Thought-Transference. After a statement and discussion of the problem of telepathy, four sets of experiments are detailed. The first consisted in guessing Lotto-Block numbers, 10 to 90. This experiment engaged the writer and one reagent, an advanced student of psychology who entertained a belief in the possibility of thought-transference. The procedure was for the experimenter to sit facing the back of the reagent, and to draw the Lotto-Blocks at random from a bag. If the block appeared with its number-side upwards, the experimenter held the number in some kind of vivid imagery, whose type was carefully recorded. At a signal the reagent indicated his impression, together with introspective details as to his mood of receptivity, his imagery, with its temporal course and spatial attributes; also the subjective certainty of his judgment. In case the block appeared with its blank side upwards, the experimenter refrained from imaging any number, and instead mused "upon an ocean scene."

One thousand experiments were conducted, but no evidence of transfer appeared in the results. They did show, however, frequent experiences of the reagent which he attributed to an objective source. Upon analysis the feeling of certainty in making judgments appeared to attach to the vividness and the behavior of the images entertained; but there was no correlation between the certainty of the guesses and the right cases.

The second experiment was guessing playing cards, and engaged 105 normal reagents, with 97 persons acting as agents. The attitude of the reagent was generally that of belief in some form of telepathic communication. A pack of forty cards, with face-cards omitted, was shuffled and cut by the experimenter, while by a throw of dice he determined whether the card cut was to be observed and imaged or not. The reagent, therefore, guessed a card which might, or might not, be in the mind of the agent. In case the card was imaged, the experimenter held in consciousness a determined attitude of will that the content should reach the reagent. The results of 10,000 guesses revealed no correspondences that were not explained by a statistical treatment, although 73 per cent. of the reagents graded certain of their guesses high with respect to certainty, 18 per cent. of all the guesses being so graded. The "inner experience" of thought-transference is therefore no unusual one. But these experiments bring no evidence either of accomplished thought-transference or "lucidity," *i.e.*, an ability to know the card cut, even if the agent does not know it. The conclusion reached is that such capacities must be very special, if they exist at all.

A supplemental set of experiments in which the reagents were ten "sensitives" brought analogous results. In 1,000 experiments no extra-normal capacity was revealed, although the "psychics" gave evidence of the semi-trance state of receptivity which characterizes their professional conduct, and they frequently regarded their impressions as issuing from an objective source. Of the five complete "hits" that were recorded, although four were with cards imaged by the experimenter, two were estimated at the lowest grade of subjective certainty, and the remaining three were attributed to "guessing" on the part of the reagent.

The "feeling of being stared at" was also investigated by ten reagents who professed a belief in such impressions and their significance. The experimenter seated behind the reagent stared at the back of his head, or refrained from so doing, according to a throw of dice. The results were again negative, so far as any objective influences could be detected; but they demonstrated that such a "feeling" could be genuinely aroused under laboratory conditions. Introspective analysis seems to show that the "feeling" in question rests upon the attribution of objective validity to commonly experienced subjective impressions in the form of imagery, sensation and impulse. The acceptance of suggestions from the total situation is also a frequent factor in determining the impression as being that of some distant influence. Unless carefully avoided, such suggestions may, of course, provide a cue which is truly informative.

Part II records a number of experiments on subliminal influences. The chief experiments employed tachistoscopic impressions of letters and digits, but peripheral impressions, the effect of corneal reflection, whispering, etc., were also tested. With the Wirth tachistoscope 15,441 experiments were performed with 118 reagents. The exposure time varied from 10σ to 2σ, nearly three quarters being made with exposures of less than 3σ. The results show some cause beyond chance working for right cases with regard to the particular character of an object, but none as regards its general character. On the whole there seems to be some experimental evidence for Bergson's "fringe of perceptions, most often unconscious, but all ready to enter into consciousness, and in fact entering in certain exceptional cases, or in certain predisposed subjects."

Part III deals with mental habit and inductive probability. A number of interesting instances are recorded showing the influence of mental habit, particularly in the choice of numbers as recorded

in census reports upon ages, the terms of criminal sentences, estimates of star magnitudes, the grading of students, etc. The application of these habits of mind in so-called thought-transference shows that human choice is never a matter of unrestricted chance. Such habits often operate to produce what upon superficial view might be regarded as very startling coincidences. A study of inductive probability also indicates that the theoretical laws of chance distributions are an adequate explanation of the empirical distributions. "Within our field of observation there is an indefinite number of series of indefinite length in constant process; the infinitesimally probable events in the aggregate of these various series may be expected to occur frequently."

Part IV records 40,500 experiments in sound assimilation with 107 reagents. It was found that in receiving speech-sounds: words, nonsense syllables and mutilated forms, the mind contributes over 85 per cent. from dictaphone records, over 60 per cent. from telephone and distant air communications, and over 25 per cent. when the sounds are heard directly across a table. The author states that "although our students could satisfactorily understand the communications through the dictaphone, the telephone and the air when the sounds came in their accustomed order, and could record all of the sounds without error, they could not hear definitely enough to identify half of the consonantal sounds through the dictaphone, a third of them through the telephone, and a quarter of them through the air." This points to the invalidity of a person's judgment that he has heard and identified "spirit voices."

Part V. consists of three contributions by Professor Lillien J. Martin, republished in whole or in part from the pages of the *Psychological Review* and the *American Journal of Psychology*. The first is a case of pseudo-prophecy, and refers to a poster suggested by a Stanford geologist which depicted the effect of an earthquake upon the Stanford arch, prior to its actual destruction in the great earthquake. The suggestion is shown to have been based upon the geologist's knowledge of the earthquake rift in that locality. The second paper deals with "local ghosts and the projection of visual imagery," and shows the prevalence of hallucinations among those who project their imagery. The third paper shows involuntary or spontaneous imagery to be a rich source for the study of the subconscious. The method is seemingly of equal value to that of automatic writing. Yet no evidences brought to light in these different ways show the subconscious to be any richer in content than the conscious at the higher level of voluntary control.

Among the appendices, the first gives tables of numerical records; the second records some experiments in long distance thought-transference which failed to bring any results not within the range of chance. In Appendix C grounds for scientific caution in the acceptance of "proof" of thought-transference are discussed. A number of well known experiments in psychical research and their results are critically examined, and the conclusion reached that "technical experience in experimental psychology is requisite both for the control of the experiments and for the interpretation of results." Appendix D is the report of an investigation with a "trumpet medium" undertaken by Dr. Coover for the California Psychical Research Society. Measurements of respiration and pulse beat of the psychic taken during the séances indicated that the "automatic," "trumpet" and "independent" voices all issued from the medium. The occurrence of "telekinetic" phenomena was likewise traced to evasive manipulations made under cover of darkness. It is of some interest to note that despite this apparent duplicity the psychic was seemingly conscientious in her attitude, and welcomed the investigation in disregard of its adverse findings. The final appendix is a catalogue of literature in the library of Leland Stanford University relating directly or indirectly to psychical research. The volume also includes an index of names and subjects.

Taken as a whole this formidable work, with its record of minute and painstaking investigations into a realm which most psychologists refuse to enter, is an achievement not to be underestimated. Although the results are uniformly negative as regards any scientific demonstration of the "occult," they will go a long way to relieve the psychologist of the imputation that his prejudice has blinded him to the existence of "psychical" phenomena. Here are elaborate experiments undertaken with unprejudiced zeal, and a frank desire to find evidences for telepathy and spiritism, if such may be had in scientific terms. The manner in which all these results point to chance variations of a natural order, rather than to external influences of supernatural origin, will be reassuring to many who have felt that possibly they were missing important facts of mentality through lack of a sufficiently sympathetic or receptive atmosphere in their laboratory work. At length we have some evidence that all the conditions of intimate "rapport," both with "normals" and with "sensitives," can be readily induced and tested by the most approved laboratory methods, and yet under

these favorable conditions, the indications of any general or persistent psychical influences were altogether absent.

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El Psicoanalisis. HONORIO F. DELGADO. Lima: Sanmarti y Ca, 1919. Pp. 58.

The book contains five chapters consisting of the ontogeny of the sexual instinct and the formation and content of the subconscious according to the psychoanalytic conception, an exposition of the mechanism of neuroses and certain psychoses, the technic of their treatment, a criticism of Freud's doctrines with suggestion for modification, and an interpretation of psychodynamics.

The author is an enthusiastic admirer of psychoanalysis and wrote the book in an attempt to spread a knowledge of it in Spanish-speaking countries. It is a creditable work, comprehensive yet concise, and clearly written. He has read very widely on the subject in several languages, but admits his personal experience is limited to a self analysis. He does not confine himself to Freud's theories alone but includes those of Jung and Adler.

"The labor of the analyst as seen from our point of view," he says, "is to unstrangle the pathogenic emotions, facilitating their different expression through speech and to impede the association of reflexes in antagonistic groups, annulling as much as possible—by the creation of a new synthesis—the source or cause of the intrapsychic discord."

The chapter on psychodynamics is perhaps the most interesting. He follows the French in considering emotion as distinct from sensory processes. Emotions are due to the blocking of sensory impulses, which do not pass on into motor acts. This emotional residuum constitutes the unconscious. Otherwise his views are largely those of behavioristic and objective psychology.

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